

The Malleability of Eyewitness Confidence: Co-Witness and Perseverance Effects

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A theft was staged 70 times for pairs of eyewitnesses ($N = 140$) who then made a photo-lineup identification. Witnesses then received 1 of 9 types of information regarding the alleged identification decision of their co-witness. Witnesses told that their co-witness identified the same person whom they had identified showed an increase in the confidence they expressed to a confederate police officer. Confidence deflation occurred among witnesses who thought their co-witness either identified another person or had stated that the thief was not in the lineup. Initial co-witness information was not mitigated by subsequent changes to that information. A second study showed videotapes of these witnesses' testimonies to observers ($n = 378$) whose credibility ratings of the testimony paralleled the witnesses' self-rated confidence. Eyewitness identification confidence is highly malleable after the identification has been made despite the fact that physical resemblance between the culprit and person identified has not changed.

If an eyewitness says "I am absolutely confident that he is the guy I saw rob the liquor store," it is rather difficult for people to accept the idea that the witness could be wrong. Numerous studies have demonstrated a close relation between the confidence expressed by an eyewitness and people's propensities to accept that eyewitness's testimony as accurate (e.g., Brigham & Bothwell, 1983; Fox & Walters, 1986; Lindsay, Wells, & Rumpel, 1981; Wells & Leippe, 1981; Wells, Ferguson, & Lindsay, 1981; Wells, Lindsay, & Ferguson, 1979; Yarmey & Jones, 1983).

It is probably quite rational to place more trust in the validity of someone's judgment when that person makes a statement with high confidence rather than with little confidence. In the case of eyewitness identification testimony, however, the observed empirical relation between accuracy and confidence is surprisingly weak. A meta-analysis by Bothwell, Deffenbacher, and Brigham (1987) indicates that eyewitness confidence accounts for about 6% of the variance in eyewitness identification accuracy. Hence, it is not uncommon in research to find eyewitnesses making false identifications with high self-rated confidence that they are correct (or, conversely, eyewitnesses being quite unsure and yet accurate in their identifications).

What does it mean when an eyewitness says, "I am totally confident that the person I identified is the culprit"? Is the eyewitness making a statement about the extent to which the identified person resembles or matches the eyewitness's memory of the culprit? Undoubtedly, some of the variance in eyewitness identification confidence is attributed to this type of similarity judgment (see Tulving, 1981; Wells, 1993). In the absence of other sources of information, similarity may play a principal role in determining an eyewitness's confidence. In cases involving real-world eyewitnessing, however, there may be many other variables that determine an eyewitness's expression of confidence.

When an eyewitness identifies someone from a lineup, the eyewitness is expressing a belief regarding the proposition that this person is the culprit. The strength of that belief is expressed through the eyewitness's expression of confidence (e.g., "I am not sure" or "I'm totally positive"). A long history of research and theory in social psychology converges on the conclusion that people's beliefs about a stimulus are influenced not only by their own view of the stimulus but also by other people's reactions to the stimulus (Asch, 1955; Sherif, 1937). Allen and Wilder (1980), for example, argue that people often reinterpret a stimulus if they find that their belief about the stimulus does not agree with others' beliefs about the stimulus. Accordingly, we argue that an eyewitness's confidence that a stimulus person is the culprit, like one's beliefs about other classes of stimuli, can be altered without changing the stimulus itself. In general, we argue that eyewitness identification confidence is readily influenced by social factors that can operate independently of perceptual and memorial processes. (See Leippe, 1980, for a general articulation of this proposition.)

The emphasis in the current research is on the effects of co-witness information on eyewitnesses' confidence in their identifications. *Co-witness information* refers to information that one eyewitness might pass to another eyewitness regarding an event that they both observed. This information might be passed in direct conversation between witnesses or through a third party.

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That third party might be a police officer, for example, who informs one eyewitness (target witness) about what the other (co-witness) had said. In the current research we are interested in the potential power of co-witness information as a tool for studying the malleability of eyewitness identification confidence.

There are several important distinctions between the current work and previous studies of eyewitness confidence. Most of the work on eyewitness confidence has been directed at estimating the magnitude of the confidence-accuracy relation. Few studies have been directed at what Luus and Wells (1994) have called the "confidence main-effect problem." We discuss this problem in the final Discussion section. The basic idea is that critical events can cause dramatic shifts in an eyewitness's confidence without affecting accuracy. We refer to these as *confidence-malleability* effects. This kind of effect has been observed in two previous experiments. Hastie, Landsman, and Loftus (1978) found that having subject-witnesses make repeated guesses served to increase their confidence in their guesses without corresponding increases in the accuracy of those guesses. In a study by Wells et al. (1981), eyewitnesses to staged thefts made identifications of someone from a photo lineup and were then either briefed on the kinds of questions they could expect under cross-examination and told to prepare themselves for cross-examination or not. Under later cross-examination, briefed eyewitnesses were significantly more confident than those not briefed. The briefing had no effect on the accuracy of the eyewitnesses. In addition, eyewitnesses who were inaccurate showed the largest inflating in confidence as a function of the briefing manipulation.

The idea that eyewitness confidence is malleable implies that it is bidirectional—not only inflatable but deflatable as well. In some other research areas, such as people's confidence in their answers to trivia questions, confidence has been shown to be deflatable (e.g., see Fischhoff, 1982). At this point, however, no studies have shown eyewitness confidence to be deflatable. Manipulations of co-witness information allow for tests of the bidirectionality hypothesis.

The concept of confidence malleability also implies that confidence is continually updated by new information rather than freezing or showing a tendency to persevere. In other words, if one piece of information inflates confidence and a later piece of information contradicts the first piece, confidence should be readjusted accordingly. Research in judgment and decision making, however, suggests that the initial information might serve as an anchor from which insufficient adjustment is made (see Tversky & Kahneman, 1986). In addition, research in social psychology suggests that beliefs often persevere even when later information clearly discredits the original basis for that belief (Ross, Lepper, & Hubbard, 1975). Hence, we predicted that initial co-witness information would affect confidence but that later discreditations of that information would not completely eliminate the impact of the initial information.

Ecological Validity

We believe that the manipulation of co-witness information is ecologically valid in the sense that co-witness information is almost certainly shared in some criminal cases and not in oth-

ers. Obviously, co-witness information cannot be shared in cases where there was only one eyewitness. Furthermore, we can imagine multiple-witness cases in which the witnesses never interact and the other legal actors (e.g., police, attorneys) do not inform a given eyewitness about a co-witness's statements to police. On the other hand, it is nearly impossible to imagine two eyewitnesses not discussing whom they identified earlier in a lineup if they happen to be roommates. Similarly, it is easy to imagine an eyewitness who makes an identification from a lineup being told later (by police, attorneys, or news accounts) that his or her identification was corroborated by another witness (or that the other witness selected a different person from the lineup). Hence, we see the initial co-witness information variable as something that varies in actual cases.

Because we see this question about co-witness information as having practical consequences in real cases, we used a staged-crime paradigm that captured critical elements of realism. Although eyewitness experiments often use live staged crimes in which subject-witnesses are caught unaware, it is uncommon to maintain the ruse of a real crime through the time of the identification task. (For exceptions, see Malpass & Devine, 1980; Murray & Wells, 1982.) We believe that maintaining the ruse throughout the identification task and obtaining confidence statements from eyewitnesses under conditions in which they believe they are giving statements to a real police officer are critical features for studying confidence malleability. We wanted to be sure that our subject-witnesses were taking seriously their confidence statements and that they stated their confidence under conditions where there would likely be consequences for the person whom they identified.

Co-Witness Information Manipulations

Our manipulations of eyewitness beliefs about the identification decision of co-witnesses were made following the eyewitness's own identification decisions. Hence, neither the identification decision nor the similarity of the identified person could be affected by the co-witness information manipulations.

We created eight conditions plus a control condition as described in the *Method* section of Study 1. One condition was corroborative in that the eyewitness was told that the co-witness identified the same person. Other conditions involved some kind of disagreement information, such as telling the eyewitness that the co-witness identified someone else. In order to test whether or not the credibility of the co-witness's identification mattered, one disagreement condition had the co-witness identifying a person who could not plausibly have been the culprit. In addition, several perseverance conditions were included to see if, for example, initial disagreement information could be reversed by subsequent corroborative information or vice versa.

Obtaining False Identifications

We held constant the accuracy of subject-witnesses' identification decisions by creating false identifications for nearly 100% of all subject-witnesses (obtained rate = 97%). This allowed us to ignore the issue of accuracy (because all eyewitnesses who were randomly assigned to co-witness conditions were inaccurate) and focus strictly on the effects of co-witness information

on confidence. The methodology for obtaining false identification rates at or near 100% derives easily from prior treatments of eyewitness identification (see Wells, 1993; Wells, Luus, & Windschitl, in press). Four elements, used in conjunction, ensured that false identifications would occur. First, the witness's view of the culprit during the crime was brief and generally suboptimal (Lindsay et al., 1981). Second, the culprit was never placed in the lineup (Wells, 1984). Third, most of the lineup members did not resemble the culprit at all, thereby making anyone who resembled the culprit stand out as an easy choice (Wells, Leippe, & Ostrom, 1978; Wells, Rydell, & Seelau, 1993). Finally, the only accurate decision (i.e., "not there") was not explicitly presented as an option, and a strong implication was planted in the situational context that the culprit was among the lineup members (Malpass & Devine, 1984). When these four elements are combined (i.e., suboptimal witnessing conditions, culprit-absent lineup, a person resembling the culprit is allowed to stand out as distinctive in the set, and the witness is led to believe that the culprit is present), false identifications are virtually assured.

Study 1

Method

Subjects, design, procedure. Subjects were 140 male and female undergraduate psychology students randomly assigned to the nine conditions of the experiment (see Table 1). Participants were tested in same-

Table 1
Experiment 1 Design

1. No information (control)
Witness receives no information regarding the identification of co-witness.
2. Same identification
Witness is told that co-witness identified the same person.
3. Different identification
Witness is told that the co-witness identified a different person (a person who looks similar to the one he/she identified).
4. Not there
Witness is told that the co-witness did not believe the suspect was in the photospread.
5. Implausible different identification
Witness is told that the co-witness identified a different person (one who looks dissimilar to his or her choice).
6. Different/same
Witness is told that the co-witness identified a different person (a person who looks similar to the one he/she identified); the experimenter later corrects that information, stating that the other witness identified the same person.
7. Same/different
Witness is told that the co-witness identified the same person; the experimenter later corrects that information, stating that the co-witness identified a different person (a person who looks similar to the one he/she identified).
8. Same/withdraw
Witness is told that the co-witness identified the same person; the experimenter later withdraws that information, stating that she is not sure whom the co-witness identified.
9. Different/withdraw
Witness is told that co-witness identified a different person; the experimenter later withdraws this information stating that she is not sure whom the co-witness identified.

sex pairs. Upon arrival at the laboratory, the experimenter explained that the study was concerned with people's ability to match voices to faces. Participants were told that they would be asked to view a set of photographs while listening to a tape-recording with the goal of trying to match a photograph to each voice they heard on the tape. The experimenter then explained that the photographs and taped interviews were provided by previous participants in the current experiment. She further explained that the task should be somewhat difficult because the tape would be played on a "voice modulator player," allegedly a piece of recording equipment that can create distortions in recorded voices, producing variations in pitch and intonation. This information was provided to enhance the believability of the ostensible theft of the equipment. Given the apparent value of this equipment, it would seem plausible that someone might steal it, and that the experimenter would be distressed at its loss. Participants were told that they would be photographed and taped while describing some of their life experiences after completing the photo-matching task. The experimenter explained that the recording equipment was set up in a room down the hall, and she instructed participants to proceed to this room, explaining that she would be along shortly.

The theft. A confederate thief awaited them in this second room. When the subjects entered the room, the confederate glanced nervously in their direction and then grabbed a large piece of equipment and hurried from the room. The event was carefully rehearsed to create the appearance of a person who was surprised by the entry of someone into a room where a theft was in progress. Ten s later, the experimenter entered the room carrying a cassette tape. She looked toward an empty table and turned a surprised gaze toward the two participants. She then began looking around the room in search of the recording equipment she had apparently expected to see on the empty table. At this point, many participants volunteered that they had seen someone leave the room with some equipment just before the experimenter arrived. If neither of the participants volunteered this information, the experimenter told them that she couldn't find the "voice modulator player." In every case, given this prompt, participants told the experimenter about the confederate's hurried exit from the room. In response to this news, the experimenter asked the participants to wait while she searched the corridor for the person who took the equipment.

The experimenter then exited the room, waited outside the door for 20 s, then reentered and worriedly remarked that she was unable to find the young woman and must now notify the department chair about the missing equipment. She then lifted the receiver from a disconnected telephone and pretended to talk to the chair about the missing equipment, noting aloud that there were two witnesses to the theft. She "listened" to the chair reply that campus police would be notified and instructed to send someone over immediately to talk to the two witnesses. To keep the participants aware of what was developing, the experimenter acknowledged what the chair was allegedly saying by restating/confirming the chair's ostensible end of the conversation in a voice loud enough for the participants to "overhear." The phone conversation developed in a way that made it clear to the overhearing participants that the thief must have been a previous participant as no one else knew that the equipment was in that room. Hence, the experimenter should construct a list of previous participants while waiting for the police to arrive.

The experimenter stated that she would put the list together right away. She noted that she also had photographs of everyone who had participated in the study, which she could include with the list of names. Again, she "listened" to the department chair instruct her to show the photographs to the two witnesses on the chance that the thief was a previous participant in the study. The experimenter agreed and added that "it must have been one of the participants in this study—after all the only people who know anything about my missing equipment are people who have taken part in this study." The experimenter then re-

placed the receiver, turned to the participants and, without acknowledging their having overheard her talking on the phone, recounted her ostensible conversation with the department chair. She explained that the thief must have been a previous participant because the only people who knew about the stolen equipment were the experimenter herself and people who had taken part in the study. She reminded participants that she had photographs of every person who had taken part in the experiment and explained that she would show them these photos while they waited for campus police to arrive.

The experimenter explained that her file of photographs of participants in the current experiment was in another room. She asked if one person would accompany her to sort through the photographs while the second person remained in the room to answer the phone if the department chair or campus police should call. The experimenter and one participant went to a second room where the photographs were allegedly stored. The experimenter rifled through a file cabinet and then commented that she must have left the photographs in her office. She asked the participant to remain in the room while she retrieved the photographs from her office.

The experimenter returned to each witness in turn with a set of six photographs of ostensible previous participants, explaining that only 6 women had previously participated in the experiment. The experimenter always claimed that she had already shown the photographs to the other participant. The effect of this was to lead each participant to believe that he or she was the second of the two witnesses to view the set of photographs. Witnesses were asked to identify the woman they had seen take the equipment from the room. For purposes of securing a high rate of false identifications, the experimenter implied that the thief must be present in the set of photographs and instructed witnesses to indicate "which of these women did it?" After recording the witness's decision, the experimenter provided the witness with one of nine types of information concerning the alleged identification of the co-witness to the event: (a) no information about a co-witness's decision; (b) an acknowledgment that a co-witness "also identified her"; (c) a statement that a co-witness "said she wasn't one of these people"; (d) an assertion that a co-witness identified a different person (in this condition, the experimenter pointed to a photo of a woman who looked similar to the person the participant identified, i.e., a photo of a woman with the same hair and eye color, hair length, height, and build); (e) an assertion that a co-witness identified an implausibly different person (with this statement, the experimenter pointed to a photo of a woman who looked dissimilar to the participant's choice, i.e., a taller woman with a larger build and longer hair of a different color and texture); (f) The experimenter first told the witness that a co-witness "identified this woman" while indicating a photograph of a woman who looked similar to the witness's choice; 2 min later the experimenter explained that she "had the photos in a different order when she showed them to the other person." She corrected the original information, stating that "the other person, in fact, identified the same person that you did"; (g) 2 min after stating that a co-witness had identified the same person, the experimenter explained that she "had the photos in a different order when she showed them to the other witness" and so was "not sure who the other person identified"; (h) The witness was first told that a co-witness had identified the same person. The experimenter withdrew this information 2 min later, stating that she "had the photos in a different order when she showed them to the other witness" and so was "not sure who the other person identified"; or (i) The experimenter first told the witness that a co-witness "identified this woman" while indicating a photo of a woman who looked similar to the witness's choice; 2 min later the experimenter corrected this information, stating that "the other witness, in fact, identified the same person you did."

Questioning by police officer. Approximately 10 min after the experimenter placed the phone call to the department chair, a confederate wearing a campus police uniform arrived. The officer explained that

she would question each witness individually about the theft. The officer continued to explain that it is now standard procedure to videotape all interviews to obtain a clear record of eyewitnesses' reports. Participants were asked to consent to being videotaped while responding to the officer's questions. All witnesses agreed. The officer asked witnesses a series of questions concerning their memories of the theft, and the final critical question in this interview was a query concerning how confident witnesses felt about their identifications. The officer asked witnesses to quantify this response by providing a confidence estimate ranging from 1 to 10, where 1 was *not at all confident* and 10 was *very confident*. At the conclusion of the interviews, participants were thanked for their participation and fully debriefed.

Results

Only 4 of the 140 witnesses did not make an identification. The analyses are based on the responses of witnesses who made an identification from the set of photographs.

A one-way analysis of variance on self-rated confidence yielded a significant between-groups effect, $F(8, 134) = 20.05$, $p < .0001$. The mean confidence ratings and standard deviations for the nine information conditions are shown in Table 2.

Newman-Keuls analyses indicated that, with one exception, every type of information produced a significant departure from providing participants with no information. Informing witnesses that a co-witness identified either the same person or an implausibly different person (i.e., one who looked dissimilar to their choice) had the effect of inflating eyewitness confidence. Reporting that a co-witness either rejected the photospread or identified a different person who looked similar to their choice produced a decrease in eyewitnesses' confidence relative to no information.

The Newman-Keuls analyses also indicated that witnesses generally persevered in the level of confidence that was induced by the original information rather than acquiring a level of confidence implied by the later "corrected" information. Consider, for example, the fact that the same/withdraw and same/different condition means did not differ significantly, but both were significantly higher than the control (no-information) condi-

Table 2
Eyewitnesses' Mean Confidence Expressed in Their Identifications as a Function of Co-Witness Information Manipulations

Condition	Mean confidence	SD
No information (control)	6.90 _a	1.17
Same identification as co-witness	8.77 _b	0.73
Different identification than co-witness	4.67 _c	1.56
Co-witness reports "not there"	3.57 _c	2.24
Co-witness identified an implausible other	7.87 _b	2.07
Different/same	4.60 _c	1.56
Same/different	8.33 _b	1.35
Same/withdraw	8.53 _b	1.19
Different/withdraw	6.13 _a	1.51

Note. Means not sharing a subscript differ at $p < .05$ using the Newman-Keuls analysis.

tion. Note also that the same/withdraw and same/different condition means did not differ from the same condition mean.

Perseverance effects were also apparent in the different/same condition as the mean did not differ from the different condition, whereas both means were significantly lower than the control condition mean. An exception to the perseverance pattern was found in the different/withdraw condition, where the eyewitnesses' confidence in that condition was significantly higher than the different condition and not significantly different from the control (no-information) condition mean.

Discussion

These data clearly show that eyewitness confidence malleability is bidirectional. In fact, when compared with the control condition, the magnitude of the effects was generally greater for disagreeing co-witness information than it was for corroborative co-witness information. We offer three possible reasons why confidence deflation effects (from the disagreeing co-witness information) were stronger than confidence inflation effects (from the corroborative co-witness information). First, this might be an instance of the more general phenomenon in which negative events elicit stronger cognitive, emotional, and social responses than do positive events (see Taylor, 1991). Alternatively, the fact that the mean confidence in the no-information control condition was 6.90 (on a maximum scale value of 10) suggests that there might be less room for upward movement (i.e., a ceiling effect) than for downward movement. It is also possible that the impact of disagreement relative to corroborative co-witness information depends on whether the eyewitness is correct or incorrect. All of our eyewitnesses were incorrect, and this might have made it easier to deflate their confidence than to inflate their confidence. It is possible that disagreeing co-witness information would have been less powerful (and corroborative co-witness information more powerful) if co-witness information had been manipulated for accurate identification eyewitnesses.

The latter interpretation suggests that it might matter whether the co-witness information itself is perceived as likely to be accurate or inaccurate. Along these lines we note that the most powerful co-witness information was the "not-there" co-witness information. The not-there response also happens to be the correct answer (i.e., it was a culprit-absent lineup) and perhaps eyewitnesses recognized at some level the truth value of their co-witness's not-there decision. Alternatively, we might expect the not-there co-witness information to have an especially large impact to the extent that this was the one possibility that they had not considered. Recall that we had led subject-witnesses to expect the culprit to be in the lineup, and therefore we suspect that they did not think much about the not-there possibility until it was made salient by the co-witness information.

It is important to note that disagreeing information per se is not sufficient to deflate eyewitness confidence. In particular, when the co-witness was said to have identified a different person but the person identified by the co-witness was an implausible selection, the eyewitness maintained high confidence in his or her own identification. We interpret this finding to mean that these confidence shifts are not due to simple, mindless conformity but rather are the product of a deeper analysis of the mean-

ing of the co-witness's behavior. We were surprised to find, however, that the implausible-different co-witness information actually elevated confidence (as opposed to having no effect relative to the control condition). Apparently, learning that one's co-witness is prone to identification error (or a buffoon) serves to elevate one's own identification confidence. This is difficult to interpret in a rational framework. One possibility, however, is that these eyewitnesses were concerned that the officer might actually believe the co-witness (who they knew was wrong). Hence, perhaps they elevated their expressions of confidence in their own identification so as to help make sure the officer would not erroneously accept their co-witness's statements. Another possibility is that the co-witness's likelihood of accuracy becomes a standard against which the subject-witness evaluates his or her own likelihood of accuracy, to wit: "Relative to my co-witness, who I know is wrong, I am much more likely to be accurate." In any case, eyewitnesses clearly made sharp distinctions among types of disagreeing co-witness information, indicating a level of processing that went well beyond merely categorizing the co-witness information as consistent or inconsistent with their own identification decision.

We found strong perseverance in three of the four conditions in which the initial co-witness information was "corrected" with subsequent information. It is important to note that perseverance effects were observed in spite of the fact that subject-witnesses did not report their confidence following the initial information and, hence, this was not a public-commitment effect (Kiesler, 1971). Instead, this effect suggests that the confidence-malleability hypothesis needs to be qualified in a way that accommodates the observation that eyewitness confidence may be resistant to continual updating under some conditions. We had assumed that the procedure we used would be conducive to updating for three reasons. First, as already noted, witnesses made no public confidence statement following the initial information. Second, the updating information was directly linked to the initial information, and in two of the four conditions the updating information was directly opposite to the initial information. Third, the updating or "corrected" information was delivered within 2 min of the initial information. A mere 2 min seems to be an insufficient amount of time for eyewitnesses to convince themselves that they were right (or wrong) and thereby become recalcitrantly high (or low) in confidence.

These perseverance effects may be related to Allen and Wilder's (1980) contention that social influence effects on opinions about a stimulus owe to shifts in the meaning attributed to the stimulus itself. Even after the social influence is canceled, the stimulus (identified person) seems to have a new meaning to the eyewitness that is now detached from the social influence that created that meaning in the first place. These perseverance effects are similar to those observed in social psychology experiments in which subjects are given false feedback regarding their performance on some task and then debriefed about the fact that the feedback was randomly determined and that their performance on the task had not even been scored (e.g., Ross et al., 1975). Following such debriefings, subjects continue to be affected by the false feedback.

These data suggest that whatever police might say to eyewitnesses soon after their identification decisions is likely to have effects that are resistant to subsequent influence. In actual cases

we would expect updating or corrected information to follow the initial information by a much longer time period and to probably involve some intervening confidence-commitment effects as well. Both of these factors (delay and commitment) would tend to favor perseverance effects for initial information. On the other hand, delay to corrected information might produce a recency effect to the extent that the initial information loses salience over time, whereas corrected information gains in relative salience by being administered immediately prior to asking the eyewitnesses how confident they are in their identifications.

It is also possible that eyewitness identification confidence is malleable for only a very brief period of time following the identification decision. In the current experiment, the corroborative versus disagreeing co-witness information was delivered immediately following the eyewitness's identification decision. Perhaps this immediacy contributed to both the robustness of our effects and the perseverance of the initial information. We strongly suspect that eyewitnesses need a bit of time following their identification to think about why they made this decision, how clear their recollection of the culprit really is, and about the possibility that they made an error. Our procedure did not give eyewitnesses time to think about these things before we delivered corroborative or disagreeing information. Hence, perhaps we caught them at an especially vulnerable time. The corrected information, although following the initial information by only 2 min, perhaps fell outside of this small temporal window of influence. This is, of course, a testable hypothesis. If it turns out that there is only a brief critical period for malleability, then there might be simple solutions to the confidence-malleability problem in actual cases, such as requiring that lineup administrators withhold from witnesses any information about their identification for at least several minutes.

One of the four conditions designed to test for perseverance effects did not find perseverance. Specifically, the different/withdraw condition produced a mean confidence level that did not differ from the no-information control condition. We are unable to explain why perseverance effects were observed in the other three conditions but not in this condition. It should be noted, however, that the means were in the direction of perseverance (6.90 in the control condition, 6.13 in the different/withdraw condition). Furthermore, as described in our second study, subjects who observed videotapes of these eyewitnesses' testimony to the police officer perceived the different/withdraw condition eyewitnesses to be less confident than the control condition witnesses. Hence, even though the perseverance effect in the different/withdraw condition was not significant for witnesses' self-rated confidence, perseverance effects were evident for outside observers of their testimony.

Study 2

The second study involved showing the videotaped interviews that were conducted by the police officer with each witness in Study 1 to a separate sample of subjects. The primary purpose was to assess the perceived credibility of the eyewitnesses as a function of the co-witness information manipulations. Previous research in which eyewitnesses to staged crimes have had their videotaped testimony evaluated by subject-jurors

has shown that the apparent confidence of a witness's testimony is highly correlated with the perceived accuracy of that witness's testimony (e.g., Wells et al., 1979; Leippe & Romanczyk, 1989). In this study, however, the source of variance in eyewitness confidence was not individual differences but rather manipulated differences across conditions. Are witnesses who were told that their co-witness agreed with their choice perceived by others to be more confident than their counterparts who were told that their co-witness had identified a different person? If so, are these eyewitnesses also perceived to be more persuasive, accurate, and believable?

In effect, the question in Study 2 was whether or not the simple statement given to witnesses in Study 1 (concerning the alleged behavior of their co-witness) would affect the perceptions of a separate set of subjects who evaluated the resulting testimony, as would be done by jurors, judges, or other legal personnel. It is important to note that the subjects in Study 2 were not privy to information about the alleged decision of the co-witness. Hence, any systematic relation between the perceived credibility of the eyewitnesses and the co-witness conditions would have to come from the interview behaviors of the eyewitnesses and their expressions of confidence.

Method

Subjects were 378 undergraduate students who participated in exchange for extra course credit. Participants were told that a participant in a previous experiment had witnessed a theft from one of the experimenter's laboratories. The experimenter explained that this witness had first identified the thief from a set of photographs and then, while being videotaped, responded to a campus police officer's questions concerning memory for the event. Participants were told that their task was to view the videotaped testimony and then complete a questionnaire concerning their impressions of the witness's credibility. The design was the same as in Study 1 in that there were nine conditions: no information, same identification (ID), different ID, not there, implausibly different ID, different/same, same/different, same/withdraw, and different/withdraw (see Table 1). Study 2 participants were never told about the co-witness information that the witnesses themselves had received.

Participants viewed the videotaped testimony on individual television monitors (1 participant per monitor and from 2 to 4 participants per session). Each participant was assigned randomly to view the testimony of one eyewitness and, after viewing the eyewitness's testimony, was asked to evaluate the testimony on eight separate measures. The first question asked subjects to indicate the extent to which they believed that the witness had made an accurate identification of the thief (called perceived identification accuracy) on a scale from 1 = *not at all accurate* to 7 = *very accurate*. This measure was of primary importance as it was perhaps the best measure of the extent to which the co-witness manipulations affected other people's perceptions of the reliability of the identification made by the eyewitness. A second central measure was the confidence of the witness on a scale from 1 = *not at all confident* to 7 = *very confident*. This measure paralleled the primary measure in Study 1 and is hereafter called *perceived examination confidence* to distinguish it from the self-rated confidence judgments obtained directly from the eyewitnesses in Study 1. Because the videotaped testimony included each eyewitness's own response to the direct question about confidence, it could be argued that there were significant demand characteristics in this setting for Study 2 participants. We agree that there is some demand here, but in actual cases eyewitnesses routinely state their confidence on the witness stand and the omission of a confidence statement would have been unnatural. Rather than thinking of any such demand as an

artifact of the experiment, we consider this to be a natural phenomenon that reflects courtroom practices.

The remaining six measures asked subjects (a) to indicate the extent to which they thought the verbal description of the thief given by the witness was an accurate one (description accuracy), (b) how detailed they found the witness's description to be (detail of description), (c) their estimates of the accuracy of the witness's testimony in general (examination accuracy), (d) how good a view of the thief the witness had during the theft (perceived view), (e) the overall believability of the witness, and (f) the witness's ability to persuade the subject-observer that he or she had a good memory for the theft (persuasiveness). All of these measures were on 7-point scales with endpoints labeled appropriately.

Results

The eight measures were highly intercorrelated; correlations ranged from .46 to .79 (all $ps < .001$). Because these measures all reflected on the perceived credibility of the eyewitness and were highly intercorrelated, the initial analysis was a one-way multivariate analysis of variance (MANOVA) across the nine conditions. The results of the MANOVA indicated reliable differences across the nine conditions, Wilks's lambda = .48, $F(7, 363) = 83.32$, $p < .001$. Univariate analyses were then performed on each of the eight measures, and the univariates were significant in each case (all $ps < .05$). The following sections examine the patterns of mean differences for these measures across the nine conditions using Newman-Keuls tests to determine which condition means were reliably different.

Perceived identification accuracy. As can be seen in Table 3, the no-information control condition differed reliably from each of the other eight conditions. Subjects who observed witnesses from the same, implausible, same/different, and same/withdraw conditions perceived the witnesses' identification accuracy to be greater than that of witnesses in the control condition. Subjects who observed witnesses from the not-there, different, different/same, and different/withdraw conditions, on the other hand, perceived them as having a lower level of identification accuracy than that of witnesses in the control condition.

Examination confidence. Table 3 displays the mean perceived examination confidence of the witnesses. The control condition was reliably higher than the not-there and different conditions as well as being higher than the two perseverance conditions that had initially indicated that the co-witness had identified a different person (i.e., different/same and different/withdraw). None of the three conditions that involved witnesses who were initially told that their co-witness had identified the same person resulted in significantly higher estimates of examination confidence than the control condition. The means in these three conditions, however, were each in the expected direction. The implausible condition witnesses, however, were perceived to be significantly more confident in their testimony than were the control condition witnesses.

Remaining measures. The remaining six measures each showed patterns across the nine conditions that largely paralleled the perceived identification accuracy and perceived examination confidence measures. Table 3 displays the means for these measures as well as the results of the Newman-Keuls analyses. Without exception, the witnesses from the control condition were given lower estimates on each measure than were witnesses from the same, implausible, same/different, and same/

withdraw conditions. Also without exception, the control condition witnesses were given higher estimates on each measure than were the witnesses from the not-there, different, different/same, and different/withdraw conditions. Unlike the perceived identification accuracy measure, however, these conditions were not always significantly different from the control condition as assessed by the Newman-Keuls analysis. In fact, for the believability measure, none of the conditions were significantly different from the no-information control condition using the Newman-Keuls test.

In general, the stability of the pattern of means across measures shows that the co-witness information manipulation in Study 1 was effective in altering the perceived credibility of the witnesses in a consistent manner across the nine co-witness conditions.

General Discussion

The confidence that eyewitnesses express in their identifications is extremely malleable as a function of what they are led to believe about the identification decision of a co-witness. The effect of such manipulations is not restricted to self-rated confidence but more generally affects the apparent credibility of the eyewitnesses' statements to police. Eyewitnesses who were led to believe that their co-witness's identification corroborated their own were perceived to be more accurate, be more persuasive, have had a better view, and give better descriptions of the thief than were eyewitnesses who were led to believe that their co-witness's identification was in disagreement. Hence, we argue that these confidence-malleability effects are likely to have implications for the impact that an eyewitness will have on others who observe his or her testimony (e.g., judge and jury).

This work has implications for our theoretical understanding of eyewitness confidence as a psychological construct. Implicit in prior work on eyewitness confidence is the idea that an eyewitness is confident in his or her identification only to the extent that the identified person seems perceptually familiar or matches the eyewitness's memory particularly well. Here, we have shown that eyewitness identification confidence can be dramatically inflated and deflated independently of these perceptual and memorial judgments. Although perceptual and memorial processes internal to the eyewitness undoubtedly affect eyewitnesses' confidence, eyewitnesses readily incorporate external information, such as co-witness statements, to decide how confident they are that their identification decision was correct.

Eyewitness confidence is extremely malleable, and this malleability can be bidirectional. Nevertheless, there appear to be limits to the extent to which confidence remains malleable. Our attempts to reverse confidence inflation (or deflation) by discrediting or changing the information that caused the inflation (or deflation) were unsuccessful. Although we are not prepared to argue that confidence inflation and deflation effects are irreversible, our data suggest that early information (immediately after the identification decision) is likely to have much greater impact than later information.

We have dire concerns about eyewitness confidence malleability in terms of what it might mean in actual criminal cases. Because the confidence that an eyewitness expresses in his or

Table 3
Observers' Perceptions of the Credibility of the Eyewitnesses' Testimony

Measure	Co-witness information condition								
	No info	Same ID	Different ID	Not there	Implausible	Diff/same	Same/diff	Same/with	Diff/with
Perceived identification accuracy									
<i>M</i>	4.16 _a	5.00 _b	3.05 _c	2.68 _c	4.85 _b	2.69 _c	5.07 _b	5.40 _b	3.55 _c
<i>SD</i>	1.44	.75	1.46	1.39	.79	1.01	.92	.83	1.23
Perceived examination confidence									
<i>M</i>	4.33 _a	5.09 _{ac}	3.41 _b	3.10 _b	5.26 _c	3.33 _b	5.09 _{ac}	5.02 _{ac}	3.98 _d
<i>SD</i>	1.39	1.27	1.25	1.12	1.09	1.31	1.18	1.09	.82
Perceived description accuracy									
<i>M</i>	4.26 _a	4.77 _{ab}	3.45 _c	3.38 _c	5.18 _b	3.39 _c	4.97 _b	4.81 _{ab}	4.13 _a
<i>SD</i>	1.01	.79	1.17	1.28	.92	.77	.85	.98	.71
Perceived description detail									
<i>M</i>	3.02 _a	3.74 _b	2.74 _{ac}	2.15 _c	4.13 _b	2.69 _{ac}	3.63 _b	3.91 _b	2.86 _{ac}
<i>SD</i>	1.44	.81	1.11	1.20	.94	1.02	.92	.84	.77
Perceived examination accuracy									
<i>M</i>	3.98 _a	4.67 _b	3.62 _a	3.53 _a	4.97 _b	3.31 _a	4.94 _b	4.83 _b	3.87 _a
<i>SD</i>	1.14	1.19	1.08	1.26	1.06	.82	.95	.84	.80
Perceived quality of view									
<i>M</i>	3.51 _a	4.40 _b	2.95 _a	2.25 _c	4.41 _b	3.00 _a	4.66 _b	4.53 _b	3.47 _a
<i>SD</i>	1.21	.96	1.02	1.16	1.11	.83	.92	.85	.98
Perceived believability of testimony									
<i>M</i>	4.70 _{ab}	4.88 _a	4.05 _b	3.88 _b	5.41 _a	3.94 _b	5.29 _a	5.06 _a	4.47 _a
<i>SD</i>	.79	1.18	1.09	1.22	.81	.93	1.02	.88	.81
Perceived persuasiveness of testimony									
<i>M</i>	3.68 _a	4.61 _b	3.14 _{ac}	2.73 _c	4.97 _b	3.00 _{ac}	4.69 _b	4.55 _b	3.32 _{ac}
<i>SD</i>	1.09	1.14	.81	1.06	1.03	1.24	.91	1.15	1.20

Note. Means within rows not sharing a common subscript differ at $p < .05$ using the Newman-Keuls analysis.

No info = no information; Same ID = same identification; Different ID = different identification; Implausible = implausible-different identification; Diff = different; with = withdraw.

her identification has been sanctioned as a reliable cue to accuracy in judicial rulings (e.g., Neil v. Biggers, 1972) and because people intuitively use confidence to judge the likelihood of identification accuracy, we argue that there is an incentive for police and attorneys to manipulate their witness's confidence. Because most eyewitnesses who view lineups are prospective prosecution (rather than defense) witnesses, we will focus on the prosecution side of the issue. The question then arises as to what police or a prosecuting attorney might choose to reveal to an eyewitness who has identified a key suspect or defendant. Would they be as ready to tell the eyewitness that a co-witness said that the culprit was not in the lineup as they are to tell the eyewitness that a co-witness identified the same person that they did? Is there an intuitive understanding by police and prosecutors that they should tell the eyewitness if the co-witness is corroborative but withhold this information if the co-witness is in disagreement? Given the power of such information on the persuasive impact of the eyewitness's subsequent testimony, is the manipulative control of such information an illegitimate form of witness tampering? Or is this merely an acceptable exercise in the adversarial justice system?

If it is acceptable to report co-witness information selectively according to whether or not it will inflate the eyewitness's confidence, would it also be acceptable to share other incriminating bits of information? What if, immediately following an eyewitness's identification of someone from a lineup, the administering officer says "You got him! That's the guy we were after. That sleazeball has been doing this stuff for months and now we've nailed him." There is nothing to prohibit such behavior by police, but it raises the question of what it really means to ask the eyewitness how confident he or she is that he or she identified the right person after the eyewitness has been told something like that. What meaning will jurors attach to that eyewitness's subsequent in-court testimony when the eyewitness comes across as highly confident that the defendant is the person that he or she saw commit the offense?

Although much of the eyewitness research literature has shown a weak relation between eyewitness identification confidence and accuracy (e.g., $r = .10-.15$), some studies have shown stronger relations (e.g., $.30-.40$). Presumably, there is some diagnostic value to asking an eyewitness how confident he or she is that he or she has identified the right person because the eye-

witness can assess to some extent how closely the identified person resembles the eyewitness's memory of the culprit. When eyewitnesses are given incriminating or exonerating information about the identified person prior to their making a confidence statement, however, their confidence is likely to undergo a robust shift that makes it largely uninterpretable as an index of how closely the identified person resembles their memory of the culprit. Under these conditions, confidence becomes confounded with what the eyewitness was told about other witnesses' behaviors or what the eyewitness was told about other evidence against the accused rather than being a clean index of how much the suspect resembles the witness's memory of the culprit.

The utility of eyewitness confidence for predicting eyewitness accuracy is presumed to be indexed by the magnitude of the point-biserial correlation between confidence and accuracy or by other statistical indexes of association (e.g., the binomial effect size display, Rosenthal, 1991). Unfortunately, the practical utility of eyewitness confidence as a predictor of accuracy may be limited in actual cases by main-effect shifts in confidence across cases or by changes in the base rate of accuracy (Luus & Wells, 1994). Suppose, for example, an experiment was conducted in which half of the eyewitnesses were accurate, half were inaccurate, the correlation between confidence and accuracy was .25, and the mean confidence on a 7-point scale was 4.5. Under these conditions, we can estimate, using Rosenthal's (1991) binomial effect size display, that the probability that an eyewitness whose confidence is above 4.5 is accurate at approximately .625 and that the probability that an eyewitness whose confidence is below 4.5 is accurate is about .375. That is a fairly impressive statistic, because we could use the eyewitness's confidence statement to improve our prediction from 50% accuracy to 67.5% accuracy by setting a cutoff score that accepts the testimony of those whose confidence is over 4.5 and rejects those below 4.5. Suppose, however, something produces a main effect shift in the eyewitnesses' confidence such that mean confidence is now 5.5, whereas both accuracy and the accuracy-confidence correlation remain constant at $r = .25$ and accuracy = 50%. Under these conditions, the previous cutoff score of 4.5 produces a distribution above that cutoff that is barely superior to ignoring confidence altogether (approximately 56% of those who have confidence scores above 4.5 are accurate).

Our general point is that the practical utility of an eyewitness's expressed level of confidence in a given case must be interpreted not only in regard to an assumed magnitude of confidence-accuracy relation but also with regard to accuracy base rates and mean confidence levels. Any time there is a shift in mean confidence without a corresponding shift in accuracy rates, a given level of confidence takes on a different meaning even if the accuracy-confidence correlation remains constant.

Although we are concerned about confidence malleability and alarmed over the size of the malleability effects that we obtained, we believe that the problem can be easily controlled in actual cases. It has been advocated elsewhere that the officer who administers a lineup should not know which lineup member is the suspect and that confidence statements should be obtained from an eyewitness immediately following the identification (Wells & Luus, 1990). This forces the eyewitness to base his or her confidence assessment on memory rather than other

information in the case (Wells, 1988). When we ask an eyewitness how confident he or she is in the identification, we want to know something about the depth of the eyewitness's memory and how closely the suspect matches that memory. When an eyewitness is asked about identification confidence, we want to receive an answer that is based on memory and perceptual judgments rather than inferences or deductions from other "facts" in the case. This recommended procedure does not prevent police or attorneys from introducing confidence-altering information at a later time, but if the initial confidence statement is recorded (e.g., videotaped), then opposing counsel can use discovery rules to introduce the eyewitness's initial confidence statement and raise questions as to why the eyewitness's confidence has undergone inflation.

Finally, it could be argued that it is rational for eyewitnesses to raise or lower their confidence as a function of co-witness information. Specifically, it could be argued that an eyewitness is in fact more likely to have made an accurate identification if a co-witness identified the same person than if the co-witness did not identify the same person. We accept this argument at some level and it is not our intent to argue that eyewitnesses are behaving irrationally if their confidence changes as a function of co-witness information. If this initial malleability is not subject to continual updating (as our perseverance data indicate), however, it becomes difficult to reconcile their behaviors with that of a rational model. Furthermore, eyewitnesses who make false identifications commonly identify the same person as their co-witnesses because each may be subject to the same forces that promote mistaken identification in that situation. In any case, confidence inflation resulting from corroborative co-witness effects is likely to be subject to double counting by jurors who might reason that there are not only two eyewitnesses who agree in their identifications but they are both highly confident as well! Hence, regardless of the issue of rationality on the part of eyewitnesses, confidence malleability is a significant practical problem for triers of fact who rely heavily on witness confidence to make important decisions affecting eventual verdicts.

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